

may appear as an Appendix to a volume of the *Monthly Notices* [the sizes of the pages in *Proc. R.S.* and *Monthly Notices* are almost the same], the extra expense of this one thousand copies being borne by this Society. Fellows of this Society may expect to receive this Appendix during the present summer for inclusion in the present volume of *Monthly Notices*.

As regards the full and final reports these will be printed as a volume of *Philosophical Transactions of the Royal Society*, and copies of this volume will be presented by the Royal Society to our Fellows who apply for them, and to such observatories, institutions, &c. as receive copies of our Memoirs.

It may be added that our Council have referred to the J.P.E. Committee the question whether it would not be desirable to invite other observers than those sent out by the Committee to communicate their results for publication along with these final reports in the volume of *Philosophical Transactions*.

H. F. Newall, }  
H. H. Turner, } *Secretaries.*

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*Reply to Dr. Rambaut's Note "On the Effect of Chromatic Dispersion."* By David Gill, C.B., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Monthly Notices* for 1898 March Dr. Rambaut communicates a long and elaborate note on a previous paper of mine entitled "On the Effect of Chromatic Dispersion of the Atmosphere on the parallaxes of  $\alpha$  Centauri and  $\beta$  Orionis."

I desire before entering on any criticism of this note to acknowledge the value of Dr. Rambaut's original paper in so far as it suggests the desirability of expressing the possible effect of chromatic dispersion on parallax researches; and indeed I have acknowledged the value of that suggestion not only in my original paper, but in the best manner in my power, viz. by adopting it. In the determinations of stellar parallax, which I have since published, I have given the effect of chromatic dispersion in terms of  $\Delta\beta$  on the result, or, when the observations admitted of it being done, I have determined the special value of  $\Delta\beta$ .

In no case, however, has  $\Delta\beta$  proved to be a sensible quantity.

The point in Dr. Rambaut's original paper, which I was compelled to dispute, was the conclusion that my results were affected by a systematic error following the law of the effect of chromatic dispersion. Had such an error existed to the extent which Dr. Rambaut's discussion professed to show (viz.  $\Delta\beta = 0''.095$ , with factors of  $\Delta\beta$  ranging from  $-2.3$  to  $+0.7$ , producing systematic errors amounting to  $+0''.2$  and  $-0''.07$ ) I should certainly have been guilty of extreme want of circumspection in having over-

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looked them ; and if such systematic errors had really existed, the gravest doubt would be thrown on the reliability of all refined determinations of solar or stellar parallax.

I believed, and I still hold, that in my original paper I conclusively proved that the corrections  $x$ ,  $x_{II}$ ,  $x_{III}$ , and  $x_{IV}$ , were due to real subjective errors, and that the true value of  $\Delta\beta$  derivable from the observations of *a Centauri* is quite insensible (viz.  $-0''.015$  with the probable error  $\pm 0''.059$ ), and that my original value of the parallax of *a Centauri* remains unchanged.

In support of his original contention, however, Dr. Rambaut proceeds to make the following statements, which I quote and designate for brevity as follows :—

- (a) "It has always seemed to me most unsatisfactory to assume empirical corrections to remove the larger residuals and then, appealing to the smallness of the resulting residuals as a proof of the accuracy of the observations, to compute the probable error of the result by means of these reduced residuals."
- (b) "It is a mistake in principle to assume the existence of constant errors unless one is absolutely driven to it."
- (c) "The fact of an error of the same sign affecting a number of observations in a series does not prove it to be a constant, and in assuming it to be so we assume not only its magnitude, but also the law of its operation."
- (d) "If we assume the existence of constant but unknown errors, and apply corrections for them, we must not ignore the fact that they are *errors*, and we must therefore include them in computing the probable error of an observation."
- (e) "The irregular errors of observation are just as *real* as the systematic errors, and until we are assured that the latter are really constant, or until we have established the law of their operation, we must treat them as being subject to the law of frequency of errors, and take them into account in estimating the probable error of the result."

Now all these statements excepting (b) are quite incontrovertible. I fully grant their truth, and only contend that neither in my original discussion of the parallax of *a Centauri* (*Mem. R.A.S.* vol. xlviii.) nor in my subsequent one (*Monthly Notices*, vol. lviii. p. 63) have I omitted to regard them.

With regard to (b), however, I would prefer to render it as follows :—

"In every refined investigation in which the conditions of observation are in any way systematically changed the astronomer must take the greatest care to ascertain whether such systematic change in the conditions of observation is accompanied by a corresponding change in the results, and should so arrange

his observations as to afford the data necessary for a complete discussion of the law of such systematic error and for the computation or elimination of its effects."

I think there are very few experienced astronomers who will deny the soundness of this statement, or who will not agree that the danger of neglecting such precaution is likely to lead to erroneous conclusions.

In replying to Dr. Rambaut's criticism I shall therefore confine myself to two points.

- 1st. That my observations were affected by true systematic errors, and that these errors were traced to their true source, viz.—to four *true* constant subjective errors due to the four different conditions of measurement.
- 2nd. That the method employed to compute the necessary corrections and the probable error of the resulting parallax was the only accurate one.

First, then, what are the conditions which distinguish true systematic errors from those which are accidental, or which distinguish between legitimate and empirical corrections?

Primarily, of course, it is necessary to regard any physical source of possible error (such, for example, as Dr. Rambaut's  $\Delta\beta$  with the factor  $\tan \zeta \cos(p-q)$ ) as a legitimate unknown quantity which may properly be introduced into the equations. But besides such obvious possible sources of error, there may be others which are personal or subjective, *provided that the conditions of observation are changed*.

It is, of course, open for Dr. Rambaut to deny that this is the case. In his statement (*b*) he expresses his unwillingness to assume the existence of constant errors unless absolutely driven to it. This seems to me a very strange statement for any experienced astronomer to make. In my opinion it rather becomes the astronomer who aspires to obtain truthful results to be continually on the outlook for sources of possible systematic error.

If Dr. Rambaut had any experience of refined heliometer observations it would perhaps be easier to convince him. If, for example, he had made heliometer observations, without the aid of a reversing prism, of the angle between a distant star (50' or 60') and one of the components of a fairly close double star at very various hour-angles, and on opposite sides of the meridian, he would instinctively feel that, in pointing on such an unsymmetrical object, he could not be certain that his observations were unaffected by personality depending on the hour-angle or on the direction of measurement with respect to the observer's eyes.

As a proof of the general existence of such an instinctive feeling, and of the necessity for optical symmetry and constancy in apparent direction of measurement in his work, every careful

modern heliometer observer has now adopted the use of the reversing prism which I first introduced in my heliometer observations of *Mars* in 1877.

Had the reversing prism been employed in the observations of  $\alpha$  *Centauri* in question much labour would have been saved, because the necessity for measures to determine the systematic corrections dependent on the direction of measurement with respect to the eyes of the observer would have been avoided.\* But I was careful to make the special observations necessary to determine these systematic errors, discussed them, and believed that astronomers generally had adopted my conclusions as sound. Dr. Rambaut has denounced these corrections as *empiric*, and comes to the conclusion that my so-called empiric corrections can in reality be traced to a true physical origin, viz. chromatic dispersion of the atmosphere. This is at least his original contention, which provoked my criticism, and he has not yet admitted his error. Let me show once more, and if possible more forcibly, that Dr. Rambaut entirely fails to prove his thesis. Dr. Rambaut says "that Dr. Gill's argument founded on a comparison of the means of the residuals in each group is of *absolutely no value whatever*, his solution being artificially constructed to fulfil the condition that these means should each be zero." The latter part of this statement is perfectly true; but I have never employed any such argument, and it hardly required nearly two pages of algebra to show the self-evident fact that my small mean residuals of the groups in the fourth place of decimals only go to demonstrate the general arithmetical accuracy of work which was computed to three decimal places only.

But in examining the accuracy of Dr. Rambaut's solution it was surely reasonable and fair that I should divide the residuals of his solution into four groups corresponding to the four circumstances of measurement. If with the introduction of his  $\Delta\beta$  in place of my  $x_i, x_{ii}, x_{iii}, x_{iiii}$ , Dr. Rambaut had succeeded in representing all these groups of equations within reasonable limits of error, then he would have triumphantly proved his point, viz. shown that my  $x_i, x_{ii}, x_{iii}$  and  $x_{iiii}$  were unjustifiable empirical corrections, and that his value of  $\Delta\beta$  was a true quantity. Unfortunately for Dr. Rambaut's argument, such is not the case.

Take the group 3, the largest group, containing 41 equations: all his residuals in this group, with three very small exceptions (viz.  $+0^{\text{R}}.007$ ,  $+0^{\text{R}}.007$ , and  $+0^{\text{R}}.005$ ), are negative, the mean value being  $-0^{\text{R}}.0139$ .

Now Dr. Rambaut finds, from his own solution, that the probable error of a single observation is  $\pm 0^{\text{R}}.0131$ ; in other words, *the mean residual in this group of 41 observations is greater than*

\* The reasons which compelled me very reluctantly to abandon the use of the reversing prism are given (*Mem. R.A.S.* vol. xlviii. p. 11), and need not be here repeated.



the probable error of a single observation!! If this fact alone does not prove the existence of a systematic error, and the neglect of a true systematic correction of some kind in Dr. Rambaut's solution, I think it will be difficult indeed to convince him! The chance that the mean residual of a group of 41 observations, unaffected by systematic error, shall exceed the probable error of one observation is about the same as that in tossing a coin "heads" will come up 41 successive times or "tails" 41 successive times.

Has Dr. Rambaut considered what are the chances of tossing 41 successive "heads"? If so, he will have determined the chances in favour of the truth of this part of his solution.

In group 4, which consists of 13 observations, Dr. Rambaut's mean residual is  $+0^{\text{R}}.0246$ , so that the mean value of the residuals of 13 observations is nearly double the probable error of a single observation!! Further comment is unnecessary.

Turning now to my own solution. Dr. Rambaut questions "the validity of Dr. Gill's method of introducing constant corrections to smooth down the roughness of the observations, unless he takes into account the hypothetical character of these corrections in estimating the probable error of the result."

Here, again, Dr. Rambaut is in principle perfectly correct, but absolutely wrong as to his description of the reasons which led me to adopt the corrections in question. Before making such a statement, he should have carefully considered the circumstances which led me to the conclusion that the corrections in question were true subjective constant errors, due to the unsymmetrical character of one of the objects under observation. These circumstances are all fully detailed in my original memoir. First of all, I was fully aware of the possible effect of change of conditions of measurement, because, as early in the series as it was possible to do so, I began to make observations in the early morning as well as in the evening, so as to determine whether any such systematic differences existed. The immediate reduction of the observations showed that such errors really existed, and there was even then strong evidence that these errors were very closely dependent on the direction of measurement with respect to the line joining the eyes of the observer, so that careful note was made of this direction in the subsequent measures (*loc. cit.* p. 17). But I was careful not to assume this to be the case. In his criticism of the "hypothetical character of these corrections" Dr. Rambaut entirely omits to note the great care which I took to ascertain whether these errors were really constants or functions of the hour-angle, viz. by making and discussing my Solution III., in which, instead of assuming the corrections to be constants, they are expressed in the form—

$$x + \cos (p-q) \alpha + \sin (p-q) \beta + \cos 2 (p-q) \gamma + \sin 2 (p-q) \delta$$

(*loc. cit.* p. 35 *et seq.*).

This solution led to a value of the parallax practically identical with that of Solution I., but with a rather larger probable error for the single observation, tending to show that the errors are more nearly constants than functions of the hour-angle. This conclusion is, however, very much strengthened if we arrange the residuals of Solution III. in order of hour-angle in the same manner as those of Solution I. are given in my paper, *Monthly Notices*, vol. lviii. pp. 59-62. Then we find that in group 1 the residuals begin at 8<sup>h</sup> 17<sup>m</sup> S.T. with a negative tendency, but become all positive towards the end of the first group; then in the immediately following group 2 the residuals suddenly change, and are chiefly negative in sign. What is the cause of this sudden change? Why, assuredly the change depending on the change of direction of measurement with relation to the line joining the eyes of the observer—there is nothing else to account for it. Passing from group 2 to group 3 there again occurs a sudden change in the sign of the residuals from negative to positive, showing that *no correction which is a function of the hour-angle will satisfy the observations!* Again in group 3 the residuals in the early part of the group are all positive, and nearly all negative towards the end, and then in the immediately following group 4 there is a sudden change towards the positive residual.

On the other hand if we take my Solution I., in which the corrections are assumed to be constants, we find that the positive and negative residuals are most fairly distributed throughout each group, notwithstanding the large range of hour-angle in each.\*

If these are not valid proofs that the necessary corrections are true constants then I am unable to understand what valid proofs are.

These proofs contrast very favourably with the extraordinary solution originally proposed by Dr. Rambaut in which in a group of 41 successive equations (the order being that of the magnitude of the factor of a term the existence of which he is seeking to prove) 38 of the residuals have the same sign, and the mean residual exceeds the probable error of a single observation; or in another group of 13 observations, in which his mean residual is double his probable error for a single observation.

Again if we take group 3 of my Solution I. we find that the factors of  $\Delta\beta$  vary from +0.18 at the beginning to -0.98 at the end of the group; but there is no evidence amongst my 41 residuals of any systematic tendency to increase or diminish from one end of the group to the other, as must necessarily be the case if  $\Delta\beta$  were a sensible quantity.

Similarly, in group 4 the factors of  $\Delta\beta$  vary from -1.08 to -2.39; and here again we find in the run of my residuals no

\* If Dr. Rambaut should reply to this paper I trust he will avoid the mistake he previously made, and note that this argument refers to the run of the residuals in each group with reference to hour-angle in the group itself, and not to the agreement of the mean residuals of the different groups.

indication in the character of the residuals which points to a real value of  $\Delta\beta$ .

Besides all this I have given the more general solution (*Monthly Notices*, vol. lviii. p. 63), in which I have introduced Dr. Rambaut's  $\Delta\beta$  as an unknown quantity, and derived its value, viz.  $-0''.015$  with the probable error  $\pm 0''.059$ . It has therefore no proved reality, and its most probable value has the opposite sign to that found for it by Dr. Rambaut.

I think it is hardly worth while to discuss irrelevant abstract statements such as "if Dr. Gill will allow me eight such corrections I can reduce the residuals so that none of them will exceed  $0^R.010$ ; twelve such corrections will reduce them all below  $0^R.005$ , &c." . . .

I must point out to Dr. Rambaut that in the discussion of astronomical data common sense and a reasonable use of the critical faculty are absolutely necessary. If there were twelve *a priori* common-sense reasons for the introduction of 12 constants in a discussion of an investigation of stellar parallax it might be desirable to introduce the whole of them. But, of course, such corrections must not be assumed without some valid reason, such as a change in the method of observation or some instrumental change, or change in some external physical condition; and it would be necessary to determine by careful discussion—such as I have given in the case of *a Centauri*—whether the corrections are real and are truly constant or demonstrably follow some other law. Dr. Rambaut would also find that the introduction of 12 unknown constants would so reduce the weight of his resulting parallax as to make it quite indeterminate.

It is true that Dr. Rambaut states (page 259), "I do not for a moment mean to imply that a solution of this sort" (the introduction of arbitrary corrections)\* "is comparable with Dr. Gill's, where his groups were arranged for him by considerations as to the position of the observer's head with regard to the line joining the stars. I merely intend to point out how misleading an appeal to the smallness of the residuals is in such a solution." I have nowhere stated that the smallness of the residuals should alone be appealed to in such a case; on the contrary, in the illustration which I gave from the discussion of my observations of  $\beta$  *Orionis* I have only introduced the unknown quantities  $\Delta\beta$  and  $\pi$ , the existence of which were physically possible, and the introduction of either or both of these quantities *reduced* the sum of the squares of the residuals, but has *augmented the square of the mean error of a single observation*.

On these grounds I held that the values of  $\Delta\beta$  and  $\pi$  were most probably insensible.

It is true that I have pointed out (page 57) that "it is a suspicious fact that the weight of an observation by Dr. Rambaut's solution is reduced to less than one half of that from my Solution I."

\* The words in parenthesis are mine, and inserted for explanation.

Dr. Rambaut tries to force this statement into an expression that my chief grounds of objection to his solution are that the residuals are increased ; whereas the real and solid ground of my rejection of his result is based on the fact that there is no correspondence whatever between his residuals and the coefficients of the term which he has introduced for the purpose of bringing the observations into systematic accord.

How does he account for the *sudden* systematic change of sign in his residuals from one group of observations to the next ? That is the real question ; that is the point which he studiously avoids, and endeavours to cover by a cloud of formulæ relating to a side issue.

These are my arguments, to which I am most anxious that Dr. Rambaut should reply ; and I ask him to state, after he has examined them, whether he still finds that his  $\Delta\beta$  has a real value approaching the probable error of a single observation. This was his original contention. If Dr. Rambaut had proved his point it would have thrown the gravest doubts on all previous investigations of stellar and planetary parallax ; as it is, I find, from every point of view, that his solution is absolutely worthless, and entirely fails to represent the observations, and that the more closely the details and results of my own discussion are examined, the more fully do they prove the truth of my original solution.

Before entering on the entirely new contention which Dr. Rambaut has dragged into the discussion, viz. that my computation of the probable error of my result is erroneous, it seems necessary to clear the ground somewhat.

The general equation under consideration is of the form

$$x + by + cz = n,$$

where  $y$  is a term depending on the time, and  $z$  a term depending on the parallax.

The point where Dr. Rambaut is in error is as to the true meaning of  $x$  in his discussion. The term  $x$  is apparently interpreted by Dr. Rambaut to mean "the true correction to the assumed difference between the two measured distances  $\alpha$  and  $\beta$  freed from parallax at the adopted epoch."

Therefore Dr. Rambaut says, if I follow him rightly, that if the observations are of four different kinds, or made under four different conditions of observation, each of which is affected by a constant error, then the equations will divide themselves into four groups of the following types :—

Type	I.	$x + a_1 + b_1y + c_1z = n_1$
	„	II. $x + a_2 + b_2y + c_2z = n_2$
	„	III. $x + a_3 + b_3y + c_3z = n_3$
	„	IV. $x + a_4 + b_4y + c_4z = n_4$



and he proceeds very elaborately to point out how the true weights and most probable values of  $x$ ,  $a_1$ ,  $a_2$ ,  $a_3$ ,  $a_4$ ,  $y$ , and  $z$  should be determined.

The initial error which Dr. Rambaut has made, and which vitiates the whole of his subsequent conclusions, is his assumption that  $x$  is a true determinable constant independent of  $a_1$ ,  $a_2$ ,  $a_3$ , and  $a_4$ . This would only be true if we could *a priori* assume some relation between the values of  $a_1$ ,  $a_2$ ,  $a_3$ , and  $a_4$ , or that  $a_1 + a_2 + a_3 + a_4 =$  a known quantity. But as no such relation is known, it is only possible to determine from these equations  $(x + a_1)$ ,  $(x + a_2)$ ,  $(x + a_3)$ ,  $(x + a_4)$ ,  $y$  and  $z$ ; for which I have written for simplicity  $x$ ,  $x_{II}$ ,  $x_{III}$ ,  $x_{IV}$ ,  $y$  and  $z$ .

In other words our four groups of observations supposed to be affected by four constant but unknown sources of systematic error cannot yield a true value of  $x$  (in the sense in which I have defined it), because the mean of all our observations (apart from terms in  $y$  and  $z$ ), or the means of our four derived values of the systematic errors, cannot be assumed to be free from systematic error. Our problem fortunately does not require a determination of  $x$ , but merely of  $x$ ,  $x_{II}$ ,  $x_{III}$ , and  $x_{IV}$ , as I have defined them. I have already, in the earlier part of this paper, shown *that within the limits of the small accidental errors of observation, the systematic errors of the four groups cannot be represented by any continuous function of the hour-angle*; they are therefore true constants, and therefore also  $x$ ,  $x_{II}$ ,  $x_{III}$ , and  $x_{IV}$  are true constants. Being true constants there is but one legitimate method of determining  $x$ ,  $x_{II}$ ,  $x_{III}$ , and  $x_{IV}$ , viz. to treat them as true unknown quantities, and to determine their values, weights, and probable errors by the usual Gaussian method of least squares simultaneously with the values, weights, and probable errors of  $y$  and  $z$ .

The results are given on page 28 of my original memoir.

I am quite at a loss to understand such statements as the following in Dr. Rambaut's paper:—"But in any case if we assume the existence of constant but unknown errors, and apply corrections for them, we must not ignore the fact that they are *errors*, and we must therefore include them in computing the probable error of an observation. This Dr. Gill has not done, and the computed probable error of his result is consequently very much underestimated. In his recent paper Dr. Gill argues with regard to these corrections that they represent real quantities, with the implication that if this is the case they need not be taken into account in calculating the probable error."

Every competent person will admit the truth of the first part of this statement, but I should be glad to be informed where I have implied that in computing the probable error of the parallax or of the unknown corrections the errors of the determinations of these corrections should not be taken into account. My original normal equations (omitting the term depending on the square of the time) were:—

$$\begin{array}{rcl}
24.50x, & + 8.04y + 6.86y' + 15.99z = +2.16 \\
+ 13.00x_{II} & + 7.99 + 8.35 + 9.32 = +0.99 \\
+ 37.00x_{III} & + 12.61 + 18.82 - 8.69 = -1.78 \\
+ 12.00x_{IV} + 2.71 + 2.53 - 6.64 = -0.62 \\
8.04 + 7.99 + 12.61 + 2.71 + 36.56 + 37.32 + 14.95 = +1.42 \\
6.86 + 8.35 + 18.82 + 2.53 + 37.52 + 47.59 + 11.11 = +0.84 \\
15.99 + 9.32 - 8.69 - 6.64 + 14.95 + 11.11 + 48.42 = +5.65
\end{array}$$

Now the coefficient of  $z$  (the term depending on the parallax) in the normal for  $z$  of these equations is 48.42; and if we neglect the errors of the determination of  $x$ ,  $x_{II}$ ,  $x_{III}$ ,  $x_{IV}$ , and  $y$ , the weight of  $z$  would necessarily be 48.42. If, in computing the weight of  $z$ , we have regard to the error of  $x$  and  $y$ , but disregard the effect of the errors of determination of the systematic corrections—in other words, if we add all the normals in  $x$ ,  $x_{II}$ ,  $x_{III}$ , and  $x_{IV}$ , and treat the normal so formed as an equation in  $x$ , we obtain  $z$  with the weight 42.

But if we eliminate  $z$  with its weight from the whole of the six normal equations, we find the weight of  $z$  to be diminished from 42 to 22, in consequence of the effect of the errors of the determination of the other unknown quantities; and this is, of course, the true weight of  $z$ .

Also in computing the square of the mean error, instead of dividing the sum of the squares of the residuals by the number of equations, the sum is divided by the number of equations *minus* the number of unknowns. So far as I am aware, there is no other legitimate method of proceeding. The method is too well known to require further discussion or justification.

In conclusion, I may briefly criticise the five solutions, the results of which are summarised by Dr. Rambaut on page 279.

- (1) Solving without regard to the systematic differences of the four different kinds of observation.

$$\pi = 0''.819 \pm 0''.014.$$

Dr. Rambaut admits that the residuals show this solution to be inadmissible, and the deduced parallax is much too large. This is pointed out on page 23 of my original memoir. On what ground is Dr. Rambaut's original solution then to be accepted, where the residuals are just as large and as systematically discordant? The comparatively small probable error of the result is due to the large algebraic weight of  $z$ .

This solution is a good illustration of the errors into which an astronomer would fall who persistently refuses to "assume the existence of constant errors unless absolutely driven to it."

- (2) and (3) Dr. Rambaut's method of solving, by which

$$\pi = 0''.769 \pm 0''.014 \text{ and } \pi = 0''.780 \pm 0''.018.$$

The initial error of this solution is, as already shown, Dr. Rambaut's erroneous assumption that  $x$  is a real quantity—which assumption forces him to solve the equation in four different groups. He appears to overlook the fact that he cannot derive the most probable values of  $x$ ,  $x_1$ ,  $x_2$ , and  $x_{111}$  by this process from a first approximation, because their resulting values depend on values of  $y$  and  $z$  which are not the definitive values of these quantities. To obtain a nearer approximation to the truth he would require to make a second approximation, employing in it the values of  $y$  and  $z$  derived from their four values combined with regard to their weights. Introducing these more approximate values of  $y$  and  $z$ , having regard to their probable errors as affecting the weights, he could make a second solution, and so on for a third and fourth approximation; whence he would finally arrive at the same most probable values of the unknowns and their true weights and probable errors, which I found by the more direct process.

- (4) Dr. Rambaut's original solution, introducing  $\Delta\beta$  and excluding personal errors,

$$\pi = 0''.780 \pm 0''.018.$$

This solution does not represent the observations any better than solution (1), which Dr. Rambaut himself condemns. The probable error of the single observation is just as great as in solution (1), and the systematic errors of the groups just as large, indeed larger.

This is the solution in which the mean residual of forty-one successive observations (arranged in order of hour-angle) is greater than Dr. Rambaut's own computed probable error of a single observation.

- (5) My own solution

$$\pi = 0''.747 \pm 0''.013$$

alone is rational and complete.

The main point of my original paper, and of this one also, however, is that the true value of Dr. Rambaut's  $\Delta\beta$  is entirely insensible, and therefore that chromatic dispersion has quite an insensible effect on my derived parallax of  $\alpha$  Centauri.

*Royal Observatory, Cape of Good Hope:*  
1898 May.

*Note concerning Diffraction Phenomena, &c.*

By H. F. Newall, M.A.

I regret that my effort to make my note (*Monthly Notices*, lviii. [present volume], p. 3) as short as possible has led Mr. Wadsworth to think that I have done injustice to his work by the appearance of wholesale criticism. I had hoped to guard against this appearance by the explicit reference which I made to "results obtained with large and small reflectors and refractors"; and I cannot think that my intention was generally misunderstood. I am only sorry that a great press of business before leaving for India last winter made me forget my original intention of sending my note in manuscript to Mr. Wadsworth.

I am unwillingly led to believe that silence on my part now would be misunderstood. But I have little or nothing to add to what I have already said, except perhaps that it would not occur to me to interpret the paragraphs in the *Encyclopædia Britannica* in the way that Mr. Wadsworth has done. The lines quoted by him cannot be regarded as the *conclusion* of a line of argument, as Mr. Wadsworth represents them to be (*Astrophysical Journal*, vii. p. 79); on the contrary they form the introduction to a very elegant treatment of a special example of illumination near the border of an image; and, though they contain statements which are not rigorously consistent, the meaning is beyond doubt clear. Lord Rayleigh is there dealing with a case in which the *scale* of the diffraction pattern does not come into consideration, and consequently there is no need to consider the focal length in the expression for the total illumination.

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*On the Actinic Qualities of Light as Affected by Different Conditions of Atmosphere.* By the Rev. J. M. Bacon.

Apart from its relation to photography, this inquiry is of importance to the astronomer as involving considerations with regard to definition, the choice of observing stations, &c.

I have been led step by step to the conclusions at which I have arrived by the following series of tests often and variously repeated:—

Using in all cases uniform samples of sensitised paper or films, and as far as possible eliminating all accidental sources of error, I have, by means of subdued and prolonged exposures, made comparisons between the action of light proceeding from grey or blue sky.

(1) After traversing a length of tube admitting no extraneous light and containing only air at the surrounding temperature.

(2) After traversing the same tube containing various admixtures of smoke and floating mote particles of different kinds.